The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Honorary.	Foreign.	Having com- pounded.	Paying £2 12s. annually.	Paying £4 annually.	Total.
December 1, 1860..	7	49	338	7	272	673
Since compounded..	......	......	+1	......	—1	......
Since elected	......	......	+6	......	+10	+16
Since deceased	—1	—1	—19	—2	—9	—32
Withdrawn	......	......	......	......	......	......
Defaulter	......	......	......	......	......	......
November 30, 1861	6	48	326	5	272	657

December 5, 1861.

Major-General SABINE, R.A., President, in the Chair.

The President stated that Mr. William Robert Sievier, who was readmitted into the Society on the 17th January, 1856, after having ceased to be a Fellow through default of payment of his annual contribution, had from a similar cause again ceased to be a Fellow in November 1860, and had applied for readmission. Mr. Sievier's letter to the Council was read, explaining the circumstances under which, during his absence on the Continent, the omission of payment had taken place. The Statute applying to the case was also read, and in accordance therewith notice was given that the question of Mr. Sievier's readmission would be put to the vote at the next Meeting.

Mr. Charles Spence Bate was admitted into the Society.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents :

> The Treasurer.
> Sir Benjamin Collins Brodie, Bart.
> William Robert Grove, Esq.
> William Hopkins, Esq.
> James Paget, Esq.

The following communications were read :—

I. " On Crystallization and Liquefaction, as influenced by Stresses tending to change of form in the Crystals." By Professor JAMES THOMSON, Queen's College, Belfast. Communicated by Professor WILLIAM THOMSON. Received October 12, 1861.

In a paper submitted to the Royal Society, and printed in the 'Proceedings' for April 25th, 1861, I directed attention in a note (page 201), to the question of how the *surface* of a bar of ice in ice-cold water, as distinguished from the interior of the bar, may, by the application of tension to the bar, be influenced in respect to tendency either to melt away, or to solidify to itself additional ice from the water ; but did not then venture to offer a positive answer. I pointed out as a matter deserving of special attention, and as affording scope for much additional theoretical and experimental investigation, the distinction between the application to ice in ice-cold water, of stresses tending to change its form, the stresses not being participated in by the water ; and the application directly to the water, and through that to the ice, of cubical or hydrostatic pressures or tensions, these being participated in by the water and the ice alike ; and I pointed out that the theory and quantitative calculation which I had originally given* of the effect of pressure in lowering the freezing-point of water, or of diminution of pressure in raising it, applied solely to effects of pressure communicated to the ice *through the water*, and therefore equal in all directions, and equally occurring in the ice and the water ; but that when changes of pressure in one or more direc-

* Transactions Roy. Soc. Edin. vol. xvi. part 5, 1849 ; and Cambridge and Dublin Math. Journ. Nov. 1850.

tions are applied to the ice as distinguished from the water, the theory does not apply in any precise way to determine the conditions of the melting of the ice, or of its growth by the freezing of the adjacent water to its surface ; and I expressed the hope that I might subsequently communicate to the Society some further developments of the subject.

On following up various considerations which had then occurred to me, I soon formed positively the opinion that *any stresses whatever, tending to change the form of a piece of ice in ice-cold water* (whether these stresses be of the nature of pressures or tensions, that is pushes or pulls, and whether they be in one direction alone, or in more directions than one), *must impart to the ice a tendency to melt away, and to give out its cold, which will tend to generate, from the surrounding water, an equivalent quantity of ice free from the applied stresses.* I came also to the more general inference that *stresses tending to change the form of any crystals in the saturated solutions from which they have been crystallized must give them a tendency to dissolve away, and to generate, in substitution for themselves, other crystals free from the applied stresses or any equivalent stresses.* In the month of May last, I tested this inference by applying stresses to crystals of common salt in water saturated with salt dissolved from the crystals themselves ; and found the crystals to give way gradually, with a plastic yielding, like the yielding of wet snow, but very much slower. The crystals, with the brine in which they were immersed, were, in the first set of experiments, placed in a glass tube, like a test-tube, and a glass piston, or rammer, fitting the tube loosely, so as not to be water-tight, was placed on the top of the salt which lay like fine sand in the bottom, and the piston was loaded with weights. The piston went on descending from day to day through spaces, which, though small, and though diminishing as the crystals became more compacted against one another, were still distinctly visible. When the rate of descent became very slow, I added more weights, and found that the rate of descent increased, as was to be expected. I afterwards procured a strong brass cylinder with a loosely fitted, not water-tight piston, or rammer, and in this I subjected crystals of common salt in their saturated brine to very heavy stresses, and thus compressed them rapidly and easily into a hard mass like rock-salt. The top surface

presented a perfect impression of the tool marks on the bottom of the piston, such as might have been made in wax. The expulsion of the minute quantities of brine remaining in pores in the salt when it has become very closely compacted, appears to be a slow and difficult proeess ; as, after the pressure had been continued for about a fortnight, I still found a slight oozing of brine from a pore which happened to exist in the side of the cylinder.

Experiments by the application of tensile stresses, or of any other stresses than those mixed and chiefly compressive ones which arise when the crystals are pressed in a close vessel by a rammer, would probably not be very easily carried out ; and I have not as yet tried any except those by pressure. I feel quite convinced, however, that melting, or dissolving, must result from all kinds of stresses tending to change of form. I think the following statement may be assumed as a general physico-mechanical principle or axiom, and I think it involves the truth of the opinion just expressed :—

If any substance, or any system of substances, be in a condition in which it is free to change its state (whether of molecular arrangement, or of mechanical relative position and connexion of its parts, or of rest or motion), and if mechanical work be applied to it (or put into it) as potential energy, in such a way as that the occurrence of the change of state will make it lose (or enable it to lose) (or be accompanied by its losing) that mechanical work from the condition of potential energy, without receiving other potential energy as an equivalent ; *then the substance or system will pass into the changed state.* The consideration of a few cases, in some of which there is not freedom for the substance or system to change its state, and in others of which there is freedom, will render the meaning of this more clear.

Gunpowder may be cited as an example of a substance in a condition *not free to change its state*, although when it is made to explode by a spark, it passes to an altered condition, and, in doing so, even gives out a great amount of mechanical work. That is to say, that *on the whole* it is more than free to change to the exploded state, or it tends so to change, but there is some kind of obstacle at ordinary temperatures, to the change, which either vanishes at a high temperature, or requires the application of mechanical work to begin the over-

coming of it. When the change is once begun, the requisite help is given to the succeeding parts by those which have gone off first.

Again, water confined in a high reservoir is not free to go to a lower one ; although a siphon, primarily filled with water, may help the parts successively over the obstacle by lending to each the requisite mechanical work in advance, which it afterwards pays to the parts which are to follow, besides that it gives out in its fall a great additional amount of power or energy applicable otherwise. Two reservoirs of water, on the same level, and having an opening between them under the water surface, would represent the case of *perfect freedom for change of state* ; and two on a level with one another, but separated by a partition, would represent the case in which no mechanical work would finally be either given out or absorbed by the change, but in which there is not perfect freedom to change, until a siphon or other means of help is applied.

A bell hung from an axle and then turned up, and left resting against a stop a little beyond its position of unstable equilibrium, is not free to go down, but a slight pull will bring it over this position and make it free to swing, which the work stored as potential energy in the raising of it from its low or hanging position, will cause it to do ; its fall till it comes to the bottom being essentially accompanied by the loss of that potential energy, as such, though not as actual energy, out of the system of which it and the earth are the two parts, and in which change of their distance asunder constitutes change of their potential energy.

If in an atmosphere of steam resting on water at its boiling temperature for the pressure of the steam ; as, for instance, in the inside of a boiler partly filled with water, and partly with steam, an inverted cup, or bell-shaped vessel, be suspended, and if it then, being full of steam, be forced down under the water, mechanical work will be imparted as potential energy to the system of which the steam and water in the boiler form one part, and the earth is the other part ; though, for brevity of expression, the work may be spoken of as applied to the steam and water. In this case there is perfect freedom for the steam forced under the water to condense and cause by communication of its latent heat the generation of an equal quantity of steam at the surface of the water under which the bell was sunk.

The occurrence of this change of state will enable the system to lose the potential energy which had been imparted to it by the submersion of the steam, or will release that energy which had been stored, and the system *will pass into the changed state* ; that is to say, a certain part of the steam will change to water, and, instead of that, a different part of the water will be changed to steam ; and this change will be accompanied by a transmission of heat from the part condensing to the part evaporating. This is all in accordance with the axiom ; and we know otherwise that it must take place, as the steam being pressed when submerged must condense and give its latent heat to the water, and that heat must generate an equal quantity of steam at the surface of the water, where the pressure is less. Thus the truth of the axiom is confirmed.

If a quantity of ice and water be enclosed in a cylinder with a water-tight piston, and if this be put into a completely closed vessel filled with other ice and water, and if the piston then be pulled with any given force and fixed in its new position (which might be done in many ways, as for instance, by the use of an axle passing air-tight through the side of the outer vessel), mechanical work will be introduced as potential energy into the system consisting of all the things enclosed in the outer vessel. But there is perfect freedom for the water enclosed in the cylinder to proceed to freeze, obtaining the requisite cold from the ice in the water confined around the cylinder and within the outer vessel. The occurrence of this change would be accompanied by the system's losing or giving up the potential energy which had been stored in it. According to the axiom, then, the change ought to occur. But we know otherwise that it must occur ; because the diminution of hydrostatic pressure in the cylinder raises the freezing-point of the enclosed water, and makes it freeze by the cold of the surrounding mixture of ice and water, which, besides, by being itself subjected to increased pressure, tends to give out cold by the lowering of its freezing-point. Thus the truth of the axiom is again confirmed.

Lastly, if a bar of ice in ice-cold water be subjected to any stress (a pull for instance) tending to change its form, it will receive mechanical work from the force, or forces, applied, and that work will be stored as potential energy in the elasticity of the ice. Now, if there be another piece of ice in juxtaposition with this piece, seeing

that, at the beginning, both these pieces were free from externally applied forces, and were both in the state in which either was perfectly free to melt and cause an equal quantity of water to freeze to the other*, it will follow according to the axiom, now supposed to be established, that the application of the stress *will cause this action to occur.*

The case of crystals in their solutions might be stated almost in the same words as the case of ice in ice-cold water : but it is to be observed that, in their case, the necessity for the translation of one chemical substance through another (the salt through the dissolving liquid), and not of heat or cold alone, causes a great slowness of the process, as compared with that of the yielding of the ice, in ice-cold water, to applied stresses.

At an early stage of the considerations which led to the opinions on the influence of stresses on crystallization and liquefaction described in the present paper, the question arose to me :—Is a spiculum or single crystal of ice, which has solidified itself in the interior of water, and is therefore not colder than the water, plastic ? Or would it, when in the water, and attached by one end, as for instance to a crust of ice lining the containing vessel, gradually bend upwards by its own bouyancy in the heavier water ? My idea is that it is not plastic. I cannot conceive of the growth of a crystal proceeding with one continuous or uninterrupted structural arrangement, if during its growth

* The supposition here assumed, however, of there being perfect freedom for either of two pieces of ice, which are immersed in the same water, and are alike free from stresses, to melt, and, by giving out its cold, to cause an equal quantity of water to freeze to the other, will probably not meet with assent at present from all, as it appears to be a prevailing opinion that water and ice in contact are *not* in a state of perfect indifference as to retaining or interchanging their conditions. It is supposed that ice has a property of tending to solidify water in contact with it, and the more so if there be ice on both sides of the water than if on only one side. Again, it is supposed that ice is essentially colder than water in contact with it, and that the water must continually be giving off heat to the ice. Both these opinions are inconsistent with the supposition here assumed. I conceive, however, that that supposition is amply confirmed by the fact that it was involved essentially throughout the reasoning, by which I was led to conclude that the freezing-point is lowered by increase of pressure, and to calculate the amount of the lowering. That reasoning led to true results and I believe it could not have done so unless the supposition were true, that when water and ice are present together their freedom to change their state on the slightest addition or abstraction of heat, or on the slightest application of mechanical work tending to the change, is perfect.

the part already formed undergoes permanent change of form, such as would be due to any plastic or ductile yielding. I think we must suppose the molecules in the interior of one crystal to be so locked into one another, by the forces of crystalline cohesion, that any one of them, or set of them, would experience a difficulty in making a beginning of the change of state from solid to liquid. I have not succeeded even in forming any clear conception of continuous crystalline structure admitting of what may be called ductile or malleable bending (that is, bending beyond limits of elasticity such as occurs in lead, copper, tin, and many other metals), and still remaining of the nature of one continuous crystal. What in soft or malleable crystals of copper or other metals, deposited in the electrotype process, may be the nature of the change of molecular arrangement induced by bending them, I cannot say ; but I suppose that, in their yielding, their crystalline structure is materially altered, and rendered discontinuous where, before, it was continuous.

In a mass of plastic ice, I incline to think that the internal melting, to which I attribute the plasticity, must occur at the surfaces of junction of separate crystals or fragments of crystals ; though probably pores formed by melting, by pressures, or by stresses, may penetrate crystals by entering them from their moistened surfaces or their junctions with other crystals. It now becomes clear, I think, that the influence of stresses affecting the ice, and tending to make it melt without there being necessarily any consequent pressure applied to the water in contact with the ice, must come to be taken into account in any theory of the plasticity of ice approaching to completeness. This view does not, however, I think, supersede the theory of the plasticity of ice sketched out by myself in former papers, but rather constitutes an amendment, and further development of it. Any complete theory of the plasticity of ice, and of the nature of glacier motion, must comprise the conditions as to fluid pressure and structural arrangement of the water and air included in the ice, and must so explain the lamination of the glacier, seen as blue and white veins. My brother, Professor William Thomson, in papers in the ' Proceedings of the Royal Society ' for February 25 and April 22, 1858, endeavoured to follow up my previously published views on the plasticity of ice with an explanation of the laminated structure, based on the same principles. The explanation he then offered, I

think, cannot fail to assist in suggesting the direction in which the true solution is ultimately to be sought for; yet I feel confident that no full and true solution has as yet been found*.

In the foregoing part of the present paper, I have shown reason why stresses applied to crystals when in contact with the liquid from which they have been produced, should be expected to cause them to melt or dissolve away. The following line of reasoning to show that stresses applied to a crystal will cause a resistance to the deposition of additions to it from the liquid, or, in other words, a resistance to its growth, will, I think, prove to be correct. When a crystal grows, the additions, it seems to me, must lay themselves down in a state of molecular fitting, or regular interlocking with the parts on which they apply themselves; or, in other words, they must lay themselves down so as to form one continuous crystalline structure with the parts already crystallized. It thus seems to me that, if a crystal grows when under a stress, the new crystalline matter must deposit itself in the same state of stress as the part is in on which it lays itself. If, then, we consider a spiculum of ice growing in water, and if we apply any stress, a pull for instance, to it while it is thin, and then fix it in its distended state, and if then by the transference† to the water beside it of cold taken from any other ice at the freezing-point we cause it to grow, which it may do if there be no other crystal of ice beside it more free than it to receive accessions, then the additional matter will, I think, lay itself down in the same state of tensile stress as the original spiculum was put into by the applied pull. The contractile force of the crystal will thus be increased in proportion to the increase of its cross sectional area. If it now be allowed to contract and relax itself, it will give out, in doing so, more mechanical work than was applied to the original spiculum during distention. Hence there would be a gain of mechanical work with-

* I have my brother's authority for stating that, although he believes the physical principles suggested in his papers here referred to to be capable of being developed into a true explanation of the phenomena, yet he considers further investigation necessary, and does not feel confident as to the correctness of that part of the explanation he offered, in which the mutual action of two vesicles in a line oblique to that of maximum pressure is considered.

† A theoretic air-engine for making such transferences of heat or cold was used in the reasoning by which I determined theoretically the lowering of the freezing-point by pressure; and the same is admissible here.

out any corresponding expenditure; or we could theoretically have a means of perpetually obtaining mechanical work out of nothing, unless it were the case that greater cold is required to freeze water into ice on the stressed crystal than on a crystal free from stress. Hence we must suppose that a greater degree of cold will be required to cause the stressed crystal to grow. The reasoning just given has been for brevity stated somewhat in outline; but I trust the full meaning can readily be made out, and that what has been said may suffice.

I wish now to suggest as an important subject for investigation, The Effect of Change of Pressure (hydraulic pressure) in changing the Crystallizing Temperatures of Saline or other Solutions of given Strengths,—as I feel sure that such effect must exist, but am not aware that it has been hitherto discussed or experimented on, and as it is intimately connected with the matters under consideration in the present paper and with subjects discussed in previous papers, which I have submitted to the Royal Society, on Ice.

II. "Determination of the Magnetic Declination, Dip, and Force, at the Fiji Islands, in 1860 and 1861." By Colonel WILLIAM JAMES SMYTHE, of the Royal Artillery. Communicated by General SABINE, P.R.S. Received October 23, 1861.

[Note by the Communicator.—Colonel Smythe is known to magneticians as having been Director of the Magnetic Observatory at St. Helena from 1842 to 1847. Being about to proceed, in December 1859, on a Government Mission to the Fiji Islands, which would require his residence there for some months, he addressed a letter to the Council of the Royal Society expressing his readiness to make any scientific observations that might be suggested to him as likely to be useful in a part of the globe hitherto so little known. The Council directed that the Committee of the Kew Observatory should be informed of the opportunity thus offered of obtaining a reliable determination of the present values of the magnetic elements at the Fiji Islands; and Colonel Smythe was in consequence supplied with the necessary instruments from that establishment.

In communicating to the Society this paper, containing the results